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## LADIES' DRESS SOCIETY.

EVERY one interested in the question of rational dress for women (and which of us—man, woman, or child—is not, directly or indirectly?) should know of the Rational Dress Society, the objects of which are:—

To promote the adoption, according to individual taste and convenience, of a style of dress based upon considerations of health, comfort, and beauty, and

To deprecate constant changes of fashion which cannot be recommended on any of these grounds.

The society seeks to promote its objects by means of drawing-room meetings, advertisements, circulating pamphlets, leaflets, &c., and also by issuing patterns which meet the approval of the committee. An annual subscription of half-a-crown constitutes membership. Mrs. E. M. King, 34, Cornwall-road, Bayswater, is the honorary secretary of the society, and those interested in the objects of the society are requested to write to her. The president of the society is Viscountess Harberton, an important letter from whom appears in our correspondence columns.

It is impossible to over-estimate the importance to all of us, of this question of rational dress for women (we use the more general and nobler word, for the movement is not intended to be limited to ladies). Men talk about it too frequently as if they were not personally concerned in the matter, yet there is not a man in civilised communities whose own individual health and constitution have not been affected by the fashions of feminine attire. But if this were not the case, all men are interested in the well-being of the women of their family,—mother, wife, sisters, daughters. If for unselfish reasons they are not (as they ought to be), there are multitudes of selfish reasons why they should be interested. What dress reform means to women, we leave women to show. As Mrs. King well remarks in her little pamphlet on "Rational Dress" (published by Messrs. Kegan Paul & Co., price sixpence), it may be questioned "whether men are quite in a position to talk about reforming the dress of women, still less to blame them for its follies; for it is chiefly they themselves who have bound this burden upon women's shoulders." Whenever an attempt has been made at rational reform, the sneers and ridicule which have prevented women from accepting the reform have come almost wholly from men. They have not contented themselves, as they well might, by ridiculing extravagancies, they have ridiculed the essential principles of rational reform. And it is to be observed that they adopt a manner in ridiculing reforms, quite different from that which they employ in laughing at the follies of fashion. The laughter at these amounts almost to encouragement; the sneers at those are generally as coarse and offensive as they are foolish.

It is comforting to find that "women reformers have no such feeling of despair as that expressed by men," but, on the contrary, have perfect faith in the remedy they offer. "We know," says Mrs. King hopefully, "that we are working on the natural lines, and upon true principles, and that these will of themselves achieve the moral, mental, and physical cure we need. We know that we are allied with the rising spirit of the age—that which manifests itself in the growing mental and physical activity of woman. We are ready to accept, appreciate, and profit by the greater generosity of [those] men who encourage and help us to obtain a like advantage with themselves in education: and to join them in their sports and healthy pleasures."

## FOUND LINKS.

BY DR. ANDREW WILSON, F.R.S.E., F.L.S.

PART VIII.

PASSING now to a very different group of animals, we find the *Tunicates*, *Ascidians*, or "Sea Squirts" (Fig. 1), to present us with certain highly interesting features for remark. These animals are usually regarded as poor relations of the shell-fish or *Molluscs*; and they exist both in a fixed and free state. The fixed "sea squirts" are tolerably common, and are found attached to shells and other objects dredged from deep water. Each "sea squirt" is a clear leathery bag, an inch or two in length. Like the old "wine-skin," it has two necks or openings (*d*, *i*). One leads into a wide throat or *pharynx* (A), the walls of which are richly ciliated, and which are perforated by numerous openings, whilst the whole throat, like that of the lancelet, is richly supplied with bloodvessels. The other opening is a door of exit. It leads from a sac or bag, called the *atrium*, into which the water used in breathing is wafted from the throat. Thus, when a "sea squirt" breathes, the water is inhaled by the mouth-opening, aerates the blood contained in the vessels of the throat, and is then sent into the atrium, whence it is discharged into the outer world. The "sea squirt's" stomach (B) opens from the throat, and its intestine in turn opens into the atrium. A heart (*l*) exists in the shape of a curious tube, which propels the blood for so many beats in one direction, and then, reversing its action, sends that fluid for so many pulsations in the opposite direction. A single nervous mass lies between the two openings of the body, and the other wall of the body itself consists largely of a material called *cellulose*, which is a

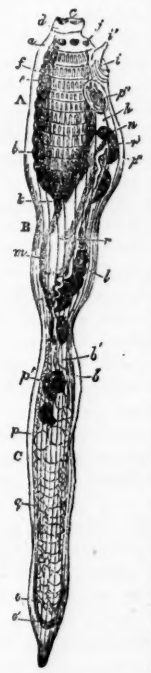

Fig. 1. *Amouroucium*, a Sea-Squirt. (A, Pharynx, or respiratory portion of the body; B, stomach; C, egg-producing organ.)

common plant compound. The animal in such a case manufactures the substance of the plant, and imitates the chemistry of the latter organism.

Such is an outline of the rooted and fixed "sea squirt." Its earlier history, however, is still more interesting. It developes from the egg in exactly the same fashion as the lancelet. The early stages of the sea squirt run parallel with those of that fish. Finally, at a certain stage, the young sea squirt makes its appearance as a free-swimming active body, somewhat resembling a tadpole, and possessing a long tail. More wonderful still, the resemblance to the lancelet has become of a closer character. A *notochord* is developed, as in the lancelet and young of other Vertebrata, and a nervous system appears above the *notochord*, as in the lowest fish. So, also, the characteristic throat, with its clefts, is formed. Now, whilst certain sea squirts appear to remain in this tadpole-stage, most of the species begin sooner or later to retrogress and to backslide in development. The young sea squirt fixes itself to some object by the suckers with which its head extremity is provided. The tail begins to shrivel, whilst the body enlarges and the throat increases in size. Then the *atrium* and the leathery skin are formed, and finally the animal becomes the bag-like organism with which we started. As a sea squirt, pure and simple, the animal loses all, or nearly all, the characters by which it was related to the lancelet; and but for the knowledge of what it *was*, we should therefore hardly know what a sea squirt (or indeed, any other animal) really *is*.

What, now, are the legitimate inferences to be drawn from the facts above detailed, and which, it may be remarked, are the common details of modern zoological instruction? Firstly, that, whilst the lancelet is the lowest Vertebrate, it shows its affinities to other vertebrates clearly enough in the possession of a *notochord* and of the other characters already detailed. Secondly, that the sea squirts are the only animals which present any likeness to the lancelet, and through it to other vertebrates—man included. Thirdly, that this likeness is so real and so close in all respects that the affinities of the two groups cannot be doubted. The likeness is seen not only in the possession of a *notochord*—found in no other Invertebrated animals—but in the curious throat with its gill-slits, and in the nervous axis placed above the notochord. Again, the likeness in the earliest stages of development is still more startling; the egg of the lancelet and that of the sea-squirt develope in precisely similar fashion. Fourthly, if these likenesses be admitted, it logically follows that they must be susceptible of explanation. The only feasible and rational account of the resemblances in question, is that which, through the aid of the theory of evolution, attributes the likeness to a common origin or descent, as it attributes the differences to modification and adaptation to special ways of life. If this explanation be admitted, it follows, fifthly, that the entire vertebrate series, from the lancelet to man, has been evolved from a root-stock represented to-day by the sea squirt's larva. The Tunicata, in this view, are the far-back progenitors of the vertebrate tribes.

One has heard frequent allusions to the so-called "base origin" of man as attributed by evolution to the sea squirt larva, and this usually from persons unacquainted with the marvellous details of parallel development in sea squirts and in the lancelet. But such persons forget that, traced back to its ultimate beginnings, the human germ itself is a mere speck of protoplasm about the $\frac{1}{120}$th of an inch in diameter, and indistinguishable in any fashion from the egg of lancelet or of sea squirt. Moreover, at a certain stage in man's early history, the developing germ resembles tolerably closely that of sea squirt and lancelet, as, later on, it has characters common to all quadrupeds, and only as an ultimate phase exhibits the special features of the human type. In a word, the hard facts of development exist, whether we like them or no; and they alone are wise who can reflect, calmly and placidly and philosophically, that these facts in no wise detract from man's place in nature; but, rightly viewed, only the more ennoble the humanity that, from such humble beginnings, has attained to the highest twig on the tree of life. Mr. Darwin's own words will bear quoting here:—"The most ancient progenitors in the kingdom of the Vertebrata at which we are able to obtain an obscure glance, apparently consisted of a group of marine animals, resembling the larvæ of existing Ascidians. These animals probably gave rise to a group of fishes as lowly organised as the lancelet; and from these the Ganoids, and other fishes, like the Lepidosiren, must have been developed. From such fish, a very small advance would carry us on to the Amphibians." Mr. Darwin's words are again worth quoting, when, in speaking of the origin of man, he remarks that "it is only our natural prejudice, and that arrogance which made our forefathers declare that they were descended from demi-gods, which leads us to demur to this conclusion (that of man's descent from lower forms). But the time will before long come, when it will be thought wonderful that naturalists, who were well acquainted with the comparative structure and development of man, and other mammals, should have believed that each was the work of a separate act of creation." And, finally, concerning the often-assumed degradation of vertebrate ancestry, the late distinguished author of the "Origin of Species" has a noble passage wherein he gives the death-blow to all arrogance of heart and mind respecting the origin of the highest forms:—"Thus we have given to man a pedigree of prodigious length, but not, it may be said, of noble quality. The world, it has often been remarked, appears as if it had long been preparing for the advent of man; and this, in one sense, is strictly true, for he owes his birth to a long line of progenitors. If any single link in this chain had never existed, man would not have been exactly what he now is. Unless we wilfully close our eyes, we may, with our present knowledge, approximately recognise our parentage; nor need we feel ashamed of it. The most humble organism is something higher than the inorganic dust under our feet; and no one with an unbiassed mind can study any living creature, however humble, without being struck with enthusiasm at its marvellous structure and properties."

In a concluding paper I shall deal with the evidence for "Found Links," furnished by a study of fossil and extinct mammals and quadrupeds.

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Speculation in electrical companies' shares is becoming very dangerous for the uninitiated. Our spirited contemporary, the *Electrical Review*, severely comments on one of these companies, namely, the "Self-Generating Electric Light and Power Company, Limited." A representative of the journal called at the office for information, and was shown a Bunsen cell and a Swan lamp. These constituted the stock. He was informed that Mr. Simon (the inventor of the company's embryo speciality) had not yet completed his lamp. Mr. Simon is, nevertheless, to receive out of a nominal capital of £300,000, the sum of £120,000. This speaks volumes. Amongst the works modestly contemplated by the company is "To manufacture bricks, quarry stone, manufacture cement and concrete, and to construct tunnels or subways for carrying wires or conductors of electricity, and to construct railways, tramways, or waterways." It is also contemplated to carry on, or at any time to cease to carry on (!!!) the business of manufacturers of electrical generators or accumulators, lamps of all kinds, &c. It is to be feared that we shall hear a deal of such companies before the year is out.

## THOUGHT-READING.

BY THE EDITOR.

WE have received from several correspondents communications on the subject of Thought-Reading, and the Willing Game as now practised in parlours and drawing-rooms. Several very remarkable narratives have been sent to us, which we have been invited to publish, along with the various theories suggested by the narrators, these theories usually resolving themselves into a vague impression that the observed results are due either to electricity or to animal magnetism.

Now, there can be no question that among the phenomena observed during these experiments there are many which are well worth scientific investigation. Even in cases where there is wilful trickery, a degree of sensibility is manifested by some among the "subjects" which is far greater than had been previously imagined, at least by those unacquainted with such remarkable instances as Dr. Carpenter and others have described in treatises on mental physiology. Take, for instance, such a case as the following, described by Prof. Barrett in the current number of the *Nineteenth Century*:—"A young lady could write words, or even rudely copy sketches which had been shown to her mother and not to herself, the mother sitting behind her and placing a finger on the girl's bare arm, even above the flexed elbow." In this case careful experiments proved that indiscernible and probably unconscious movements of the touching finger served to convey a sufficient guidance to the girl's delicate skin and quick intelligence. But no one who has examined such cases as this, and recognised the wide range of difference between one person and another in sensibility to slight muscular impressions, can attach any weight to the customary protestations in drawing-room experiments. On the one hand, the person guiding asserts that there has been no guiding, and probably often believes that there has been none; on the other, the person guided is as ready to asseverate that there has been no guiding influence whatever (and possibly may have recognised none). But experiment shows that there has been.

Thus, we must not be expected to find space for accounts of remarkable cases of apparent mind-reading or mind-guiding,—generally sent without any of the details that have scientific value, and without any attestation more satisfactory than some remark that the writer *knows* there was no trickery. Nor can we admit, as a scientific explanation, the expression of a belief that there must have been magnetism. As Professor Barrett remarks, the explanation "It is magnetism," seems perfectly sufficient "to many who, for a thousand pounds, could not write down a single true sentence on the ascertained laws of magnetic attraction;" while, "if one ventures euphemistically to suggest this, they usually take refuge in 'animal magnetism,' a phrase so obviously ordained by Providence as a safe retreat, that it would be brutal to drive them to bay on it."

But there is room for scientific research into this matter. Some of the experiments already made under suitable test conditions have led to results so remarkable, as to show that the subject is well worth examining closely. Professor Barrett, Mr. Edmund Gurney, and Mr. Frederic W. H. Myers have been at some pains to collect evidence which is, indeed, as yet incomplete, but still seems to indicate a real power of mind on mind which, to say the least, has not yet been explained. They attach somewhat more weight, relatively, to the mind-reading side of the question than I am disposed to do. It seems to me the real point to be attended to is the power of mind in guiding mind, not the aptitude of some minds to be guided—though, of course, both are parts of the same subject of inquiry. There are cases in which mind-guiding seems the prominent feature: others, in which mind-reading seems chiefly in question; others, in which it is doubtful whether one or the other has been principally at work.

Take, for instance, the following case described by Dickens in a letter to Forster. Dickens is here speaking of a professional conjurer, and therefore trickery may have, or rather must have, the fullest possible influence assigned to it; but trickery will not explain the mental phenomena. Dickens speaks of the man as "a perfectly original genius, putting any sort of knowledge of legerdemain such as I supposed that I possessed at utter defiance":—

"You are to observe," says Dickens, "that he was *with the company*, not in the least removed from them, and that we occupied the front row. He brought in some writing-paper with him as he entered, and a black-lead pencil; and he wrote some words on half-sheets of paper. One of these half-sheets he folded into two, and gave to Catherine [Mrs. Dickens] to hold. 'Madame,' he says aloud, 'will you think of any class of objects?' 'I have done so.' 'Of what class, madam?' 'Animals.' 'Will you think of a particular animal, madam?' 'I have done so.' 'Of what animal?' 'The lion.' 'Will you think of another class of objects, madam?' 'I have done so.' 'Of what class?' 'Flowers.' 'The particular flower?' 'The rose.' 'Will you open the paper you hold in your hand?' She opened it, and there was neatly and plainly written in pencil: *The Lion. The Rose.* Nothing whatever had led up to these words, and they were the most distant conceivable from Catherine's thoughts when she entered the room."

Unless we suppose that by some amazing feat of legerdemain the conjuror, after Mrs. Dickens had named the rose and the lion, substituted for the paper in her hand one on which he had written these two words, doing this in the momentary interval between her naming the rose, and opening the paper in her hand, we must suppose that he influenced her mind in some way (the determination of which is what science yet has to seek) to think first of a lion, then of a rose. The interpretation of the trick as a feat of *legerdemain* is, of course, quite out of the question. There were thousands of objects of which Mrs. Dickens might have thought first, thousands of which she might have thought next: therefore millions of combinations of two objects of which she might have thought. The conjuror could not possibly, then, have had ready to hand, among a multitude of papers, one containing in right order the two Mrs. Dickens had selected. He could not possibly have written those two names on a piece of paper in the moment between her answering "the Rose" and opening the paper in her hand at his request. Still less could he have combined (in this momentary interval of time) the accomplishment of this feat, with the extraction of one paper from her hand and the substitution of another, without any knowledge of the change either on her part or on that of the audience, including such a keen observer as her husband. It seems certain then that the conjuror guided her mind by will power to think of the objects whose names he had already written on the paper.

*(To be continued.)*

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TERRIFIC THUNDERSTORM AT BERLIN.—A terrific thunderstorm visited Berlin on Monday, the 29th ult. From half-past two to half-past three the fire brigade were summoned no fewer than twenty-eight times to extinguish fires caused by lightning, but there were no serious conflagrations. The storm was confined to Berlin and its immediate neighbourhood.

XUM

# CRYSTALS.

BY WILLIAM JAGO, F.C.S., ASSOC. INST. CHEM.

No. IV.

ANOTHER group of rocks is that in which the structure is wholly crystalline; they contain no glassy base, and often the crystals are sufficiently large to be readily discerned by the naked eye. From the very fact that the crystals are so packed and crowded, it often follows that their shape is not perfect; those last formed must of necessity fit themselves into the spaces left for them. Fig. 1 is a microscopic study of a rock of this description. This particular specimen at one time formed a part of Cleopatra's Needle, and thus possesses some little interest additional to that it derives from its geological character. This rock, usually termed "Syenite," is essentially composed of the minerals felspar and hornblende; the specimen before us also contains quartz.

Fig. 1.

Hornblende is a mineral of a very dark colour; and even in thin sections is almost opaque; but little of it is shown in the figure. There is, however, a small piece in the upper part of the field, represented by very dark cross shading. To the left is seen a space which, save a few straight lines, is almost free from markings; this portion of the rock is quartz. Starting from the bottom of the figure, and occupying the whole of the centre, is a portion of a crystal of felspar, approximately square in section. The felspar of Cleopatra's Needle is of special interest to the geologist from some peculiar markings it shows when viewed by polarised light. These consist of a series of striations, across the crystal (from left to right of the figure), but as they are most brilliantly coloured, it is impossible to represent them in a plain black-and-white sketch. To us, who are at present engaged in the study of crystal life and growth, there is one lesson in particular the felspar crystal teaches us. It may be noticed that the crystal is cloudy, instead of being clear like the quartz; this cloudiness has developed very fully in the upper right-hand corner of the crystal, where, in the figure, it is represented by a sort of stippling effect, but the whole crystal is permeated by it, although to a lesser extent. From a comparison of different rocks it is found that the felspar of some are almost clear, while in others the mineral is entirely changed. Those which have been most subjected to the action of "weathering," have suffered most. Rain, frost, and last, but not least, the atmosphere, gradually do their work, and slowly but surely demolish the crystal edifice, so laboriously built up by Nature in her constructive moods. Felspars are particularly liable to decomposition; the lime and potash or soda they contain are dissolved out by water containing carbon dioxide in solution, and leave behind a form of silicate of alumina, to which the name of kaolin has been given. This substance will be more familiar to many as "china clay." It is to this kaolin that the cloudy nature of the felspar crystals is due, and in but few rocks is the felspar entirely free from it. Not only, then, have we crystal growth and life, but these are followed by crystal decay, and ultimate decomposition. Water, the grand agent of geological denudation, not only does its work in the form of the beating wave and the rushing torrent, but also acts quietly on a gigantic scale on the constituent molecules of rock matter, and reduces them to the fine mud and sediment from which are again built up our vast formations of sedimentary rocks.

It is well known that sedimentary rocks, after a time, become much altered in appearance and properties. This is particularly noticeable in those strata which, lying deep beneath the surface, are subjected to enormous pressure and a considerable degree of heat. Although the rock may not be melted, still the heat favours in a remarkable manner the tendency of the particles to assume once more the crystalline form.

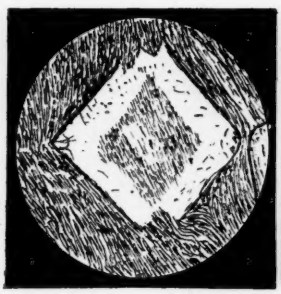


Fig. 2.

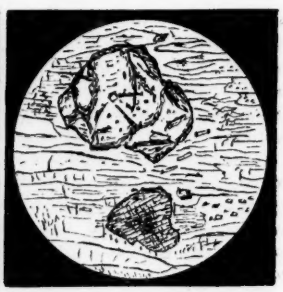


Fig. 3.

Fig. 2 represents a piece of slate from Bavaria, which has been altered by the intrusion of some eruptive rocks. Under the heat produced, crystallisation has set in, and the slate contains a quantity of crystals of a mineral known as chiastolite. The section of one, almost at right-angles, is figured. This particular specimen tells us much of the growth of the crystal. We notice that it has succeeded in thrusting the grain of the slate aside where it met it perpendicularly, but where the growth has been in the line of cleavage of the slate, the slate has held its own; two opposite corners of the crystal are thus imperfect. Fig. 3 is a sketch of rock called "granulite." It is a highly-metamorphosed, sedimentary rock; the whole mass is crystalline, the crystals being arranged, however, in regular lines, thus furnishing a clue to its origin. The two larger masses drawn are imperfect crystals of garnet—a plentiful mineral in rocks of this type.

The rocks have afforded us some striking examples of crystals—their growth and decay. One last lesson they

may teach us: Nature's methods are always those of law and order, nothing is done at random. In greater matters this is self-evident, and in subjecting her work to the closest scrutiny we ever find her faithful to herself.

## ENGLISH SEASIDE HEALTH-RESORTS.

BY ALFRED HAVILAND, M.R.C.S., F.R.M.C.S. Lond.

CLASSIFICATION (*Continued from page* 40).

### THE DAILY RANGE OF TEMPERATURE ACCORDING TO LATITUDE.

THERE is no more important element in climate than the range of temperature; it is the element by which we distinguish *continental* from *insular* climates; and the facts relating to this range enable us to deduce laws, so well formulated by Haughton,* viz:—

1. *In the northern hemisphere, places which enjoy a* maximum *mean annual temperature have also a* minimum *range of annual temperature; i.e., they have an Insular climate.*

2. *In the northern hemisphere, places which possess a* minimum *mean annual temperature have also a* maximum *range* of annual *temperature; i.e., they have a Continental climate.*

3. *Large masses* of land, *anywhere,* increase *the range of annual temperature;* and *large masses of* water, *anywhere,* diminish *the range of annual temperature.*

Now, although the climate of England is eminently *Insular*, yet for our present purpose we may, for the sake of comparison, divide it into *inland* and *coastal*: the former representing the *Continental*, and the latter the *Insular* division of the English climate as a whole. The sea-coast is not resorted to altogether for the sake of the pure sea air, that those who reside in inland towns so enjoy during their outings and search after health. There is yet another reason, unfortunately felt by many in weak health, and that is the necessity of avoiding the extremes of heat and cold, and the greater range of temperature, which are experienced in the inland climate. In summer, the sea moderates the heat of the coast line, and in winter tempers its cold; the more perfectly, therefore, these conditions obtain, the more perfectly will the climate of the health-resort fulfil the requirements of those who are anxious to benefit by its influences.

This can easily be illustrated by referring to the records of the daily range of temperature at the following inland and coast stations for 1881:—At Salisbury, by Thomas Challis, Esq., of Wilton House; at Stockton, near Rugby, by the Rev. W. Tuckwell; at Gloucester County Lunatic Asylum, by E. Toller, Esq., M.R.C.S., F.M.S.; at Brighton, by E. F. Sawyer, Esq., F.M.S.; at Llandudno, by James Nicol, Esq., M.D.; at Scarborough, in Dr. John W. Taylor's (M.O.H.) Annual Report for 1881; at Liverpool, by John Hartnup, Esq., F.R.A.S.; at North Shields, by Robert Spence, Esq.; and at Lowestoft, by S. H. Miller, Esq., F.R.A.S., F.M.S.

During the *first* quarter of 1881, the *greatest* range of temperature was observed at Salisbury, 15°·5; Stockton, 14°·9; Gloucester, 14°·7—*mean*, 15°·0. The *least* at Scarborough, 7°·4; Llandudno, 8°·6; North Shields, 8°·7; mean, 8°·2; difference, 6°·8.

During the *second* quarter, the *greatest* range was at Salisbury, 26°·3; Stockton, 22°·4; Gloucester, 22°·5; *mean*, 23°·7.

The *least* at Scarborough, 11°·8; Llandudno, 12°·1; Lowestoft, 12°·3; *mean*, 12°·0; difference, 11°·7.

During the *third* quarter, the *greatest* was recorded at Salisbury, 22°·8; Stockton, 21°·3; *mean*, 22°·0.

The *least* at Llandudno, 9°·7; Liverpool, 10°·4; Scarborough, 11°·8; *mean*, 10°·6; difference, 11°·4.

During the *fourth* quarter, the *greatest* range was at Salisbury, 17°·7; Stockton, 14°·0; *mean*, 15°·8.

The *least* at Scarborough, 7°·9; Llandudno, 9°·3; at Brighton, 9°·1; *mean*, 8°·7; difference, 7°·1. If we compare the above figures with the mean daily range of temperature throughout England for the four years 1878–1881 inclusive, we shall see at a glance how natural a division there is between the *coastal* and *inland* climates.

| | All England—1878-1881. Mean Temperature. | Mean Daily Range. | Greatest Daily Range. Inland—1881. | Least Daily Range. Coastal—1881. | Difference. |
|---|---|---|---|---|---|
| First Quarter | 38°·7 | 11°·3 | 15°·0 | 8°·2 | 6°·8 |
| Second ,, | 53°·1 | 16°·3 | 23°·7 | 12°·0 | 11°·7 |
| Third ,, | 58°·3 | 15°·4 | 22°·0 | 10°·6 | 11°·4 |
| Fourth ,, | 42°·5 | 11°·7 | 15°·8 | 8°·7 | 7°·1 |
| Annual Means | 48°·1 | 13°·6 | 19°·1 | 9°·8 | 9°·3 |

Again, referring to the first law of climate above as to the *highest* mean annual temperatures being associated with the *least* range of temperature, we have the following figures to show how this is exemplified in England, when the *inland* and *coast* climates are compared according to latitudinal position. The temperatures belonging to the interlatitudinal space, 50° to 51° N., were all recorded at coast stations, and we have selected Ventnor as the representative of this space, simply from its central position and its separation from the mainland of England.

| Parallels of Latitude, North. | All England, 1881. Mean Temp. of the Air. | Mean Daily Range of Temp. | Coastal Representatives, 1881. | Mean Temp. of the Air. | Diffrnce. in favour of Coast. | Mean Daily Range of Temp. | Diffrnce. in favour of the Coast. |
|---|---|---|---|---|---|---|---|
| deg. | deg. | deg. | | deg. | deg. | deg. | deg. |
| 50–51 | 49·1 | 13·5 | Ventnor..... | 49·2 | +0·1 | 11·4 | −2·1 |
| 51–52 | 48·1 | 15·7 | Barnstaple ... | 50·5 | +2·4 | 13·4 | −2·3 |
| 52–53 | 46·9 | 15·3 | Lowestoft ... | 47·6 | +0·7 | 11·8 | −3·5 |
| 53–54 | 46·4 | 12·5 | Llandudno ... | 48·6 | +2·2 | 10·1 | −2·4 |
| 54–55 | 45·4 | 13·5 | Scarborough.. | 47·1 | +1·7 | 9·5 | −4·0 |

Thus we see that the coastal representatives have both a maximum mean annual temperature and a minimum mean daily range of temperature in accordance with the above proposition. We see also that the *greatest* daily range of temperature takes place, both inland and on the coast, during the second quarter of the year (or during April, May, and June).

We find the sea, as it were, postponing the effects of summer and autumn, and giving out its store of heat during the last months of the year, the mean temperature of which is 42°·5, compared with 38°·7 during the first quarter. On the other hand, it absorbs in spring and early summer a more than fair proportion of the solar heat. Hence it is that many seaside health-resorts afford a warmer climate up to January, and even February, than inland places, and many persons, on this account, go to them from large towns to enjoy their prolonged warmth and equability of temperature. On the other hand, after the winter has destroyed the last lingering trace of the previous summer heat, and chilled the sea and the land, then it is that invalids feel the want of shelter from cold winds during March, April, and May, and those resorts become the most frequented which afford the greatest protection from these winds during this most critical period of the year, when more fatal chills are contracted than at any other time—more latent seeds of consumption excited into activity and development than during the cold of winter. Hence it is that we find on all the more exposed parts of the coast of England a high mortality from this disease; but this subject must not be anticipated, as it will be dealt with in its proper place.

* "Six Lectures on Physical Geography," by the Rev. Samuel Haughton, M.D., F.R.S. Dublin University Press Series.

XUM

## THE EYES OF SCIENCE.

By the Editor.

THE telescope, the microscope, and the spectroscope give to the student of science what may be regarded as three kinds of visual power—in one case unlike the visual qualities possessed by the natural eye, in the other two surpassing these greatly in degree. We can conceive creatures endowed with the powers of vision which the telescope and the microscope artificially supply. Indeed it is by no means incredible that in other worlds than ours creatures may exist possessing powers akin to these. And although it is not easy to conceive the sense of vision so increased and extended that by means of it the analysis of light effected artificially by the spectroscope could be effected naturally, yet there is nothing absolutely outside the range of possibility, even in this. The eye is, indeed, an optical instrument, precisely as the ear is an acoustical instrument; and so far as we can judge, the sense of vision might have been provided with a more complex organ, or series of organs, giving it greater range, as in the telescope, or more complete power of magnifying minute details, as in the microscope, or the power of separating light-rays of different refractive nature, as in the spectroscope. There are other optical instruments also whose powers might have had their analogues in the organ of sight (as the polariscope and similar instruments); while there are others, as the stereoscope and so forth, which, like the telescope and microscope, are akin to the organ of vision, but give to it increased power in particular ways.

I have lately been led to notice how certain photographic processes and methods extend the powers of human vision, and enable us to see what, owing to certain peculiarities in the circumstances under which eyesight is employed, we are debarred from seeing in the ordinary way.

It has long been noticed that photographic vision, so to describe this method of studying natural objects, has one great advantage over ordinary vision in that it is not liable to ordinary misleading influences. In science, seeing is not always, or even generally, believing—for the simple reason that the student of science cannot always be certain what he really sees.

Thus an observer may be misled by imagination, especially if some favourite theory has possession of his mind. If he knows, or thinks he knows, what he ought to see, or might fairly expect to see, he is very apt to imagine that he actually does see it. In this way, for instance, many students of astronomy have fancied they have seen a small companion by a star in a position where they had been told such a companion existed, when, in reality, there had been some error in the description, or in their reading of it, and either no such companion existed, or else it was in some entirely different position, and perhaps quite beyond the range of the telescope employed by the observer.

Again, the eye is repeatedly deceived by effects of contrast. Thus, the French astronomer Chacornac advanced a very ingenious—indeed, masterly—theory in explanation of the circumstance that the disc of the planet Jupiter is brighter near the edge than in the middle, the only objection to his theory residing in the circumstance that the disc is darker, not brighter, near the edge, though to the eye it appears brighter there by contrast with the dark background of the sky on which it is seen projected. So again there is a charming theory, in vogue to this day among many students of the moon, explaining why the floor of the lunar crater Plato (the Greater Black Lake of the earlier telescopists) grows darker as the sun pours more light upon it (rising higher in the sky as supposed to be viewed from Plato), the real fact being that there is no such darkening, the apparent difference being entirely due to effects of contrast—the contrast of the floor with the black shadows of the crater-ring thrown upon it when the sun is low, and the contrast of the floor with the brilliant white of the surrounding crater-ring when the sun is high, one contrast making the floor look lighter than it is, while the other makes it look darker. I may cite another instance of an optical illusion, caused by an effect of contrast—a case not requiring telescopic observation for its recognition. If on a moonlit night one looks beyond a water horizon towards the part of the sky below the moon, that region looks darker than the parts of the sky on either side; yet, in reality, it is no darker—if anything slightly lighter. What causes it to look darker is the apparent* brightness of the part of the water just below the moon, where lies seemingly* a broad track of silver light. If this track of light is concealed in any way, as by holding up a sheet of card or paper, the portion of the sky immediately above is at once seen to be at least as bright as the parts of the sky on either side of it. So in multitudes of other cases—some familiar, some otherwise—the eye is deluded by effects of contrast.

Photography, or what may be called photographic vision, is not, it is true, altogether free from defects corresponding to such defects of vision (resulting in illusion) as we have just considered. As there are physiological illusions in ordinary vision in such cases, so are there in certain applications of photography, physical effects which may prove similarly illusive. For instance, there is what is sometimes called photographic irradiation, when around a dark object in a photograph a ring of light is seen, or around a bright object a ring of darkness, this ring not corresponding to any really existent object, but resulting from some change in the photographic film along the border-line around a region acted on very strongly by light.

Again, the photographic eye has long been justly valued for its artistic power, in being able to record, without defect or exaggeration, what it sees. If we take, for instance, one of Dr. Rutherfurd's photographs of the solar disc, and compare the spots there depicted with those shown even in the most carefully-executed pictures of the sun before and since, we see at once how liable the eye is either to be deceived in what it sees, or else to fail duly to guide the hand in reproducing what the eye has seen. I happen to know of a case where a draughtsman took exceptional pains to reproduce, without exaggeration, the aspect of the solar disc with its spots, when yet, on comparison being made with a photograph taken nearly at the same time (though 3,000 miles away), it appeared that the spots had been notably exaggerated. I refer to the drawing of the sun's face which forms the frontispiece of my treatise on The Sun. I was particularly struck by the aspect of the sun when that drawing was made, and I certainly spared no pains to delineate the spots correctly; but a comparison of my picture with a well-known photograph by Ruthurfurd, which chanced to be taken about the same time in New York, will show that though the spots are delineated, individually, correctly enough, they are considerably too large as compared with the solar disc—an enlargement by no means necessary to enhance their importance, for the largest spot visible on that occasion had a surface several times larger than the entire surface of this earth.

* I say "apparent" and "seemingly" because the moon's rays really illuminate the region which appears dark, as brightly as the rest. It is only because of the position of the observer's eye that one region appears brighter than the rest.

Other instances of the same sort may readily be cited. A very noteworthy case, in which the imperfection of ordinary vision and ordinary methods of delineation caused many men of science to be long led astray, till photography finally came to their rescue, is that of the solar corona. The pictures of the corona which used to be drawn by different observers of the same eclipse, often by observers stationed within a few yards of each other, showed such discrepancies as to suggest to some the theory that the corona is not really a solar appendage, but an optical phenomenon, caused by the passage of the sun's rays through our own atmosphere; and although a very slight knowledge of mathematics sufficed when applied (for many mathematicians, failing to apply their knowledge, were long misled) to show the erroneous nature of this theory, it was not until photography had been employed to delineate the corona, that the groundlings were convinced on this comparatively simple point. A singular illustration of the inferiority of the unaided vision in this matter was given once at a meeting of the Astronomical Society. "Two people," said Mr. Stone, referring to the eclipse of 1875, which he had witnessed in South Africa, "were asked to make drawings of the corona, and at the end of the time one man had drawn it in one shape, and the other as different as it could possibly be." Considerable amusement was caused by Mr. Stone's sketching two absurdly dissimilar pictures on the blackboard. "The person who drew one picture was an engineer, and was sitting side by side with the other. Before the eclipse was over he turned round to look at the other's drawing, and said, 'What on earth are you doing here?' He replied, 'I am drawing the outer corona.' He said it was nothing of the kind; yet he looked and saw the outer corona just as the other had drawn it; but he had overlooked that it was the inner corona he was drawing, until his attention was called to it in that way."

On the same occasion Mr. Stone pointed out another defect of ordinary vision, as an instrument of scientific research, which is well worth noticing. He said very truly, that as soon as any feature has arrested the attention, it becomes prominent; another person may not catch the same feature, and, passing it over, seizes upon some other point and brings it out more strongly. Photography has no weakness of this kind, at least where due care is taken to use the same kind of plates and instruments, nearly equal in size and similar in quality, when comparison is to be made between pictures taken by different observers or at different stations.

## PHOTOGRAPHY FOR AMATEURS.

BY A. BROTHERS, F.R.A.S.

### PART X.—THE GELATINE PROCESS.

THE photographic world is chiefly indebted to Mr. Kennett for this process. In it we have one of the most simple and perfect of any of the photographic processes. The time of exposure in ordinary practice is reduced to at least one-tenth of what was required for wet collodion. This degree of rapidity may be greatly exceeded, but the beginner will find the ten-times plates quite rapid enough for ordinary work.

The first thing to attend to is to see that the room in which the plates are to be manipulated is sufficiently dark. There are many ways of effecting this. If the room have ruby glass in the window, one or two thicknesses of yellow paper as an extra covering may be sufficient. Daylight may be altogether excluded and the work conducted by artificial light. Lamps are sold for the purpose; or one may be made by cutting the neck and bottom from a hock bottle, which only requires a suitable stand to admit air, and a cover for the top, to stop the direct light of a candle or small paraffin lamp placed within. There may be plenty of light in the room, provided it be of the proper quality.

It is important that the dark slide containing the sensitive plate be covered with a black cloth when carried into the daylight, as the smallest hole or crevice in the slide or camera would admit sufficient light to *fog* the plate. The plates may be developed by the ferrous-oxalate method, or with pyrogallic acid, and a bromide and ammonia.

Instead of holding the plate in the hand while developing, as with wet collodion, a shallow dish is used, which need be only just a little larger than the plate, in order to economise the solutions. Three dishes will be necessary—one to develope in, one to contain solution of alum, and the other for fixing.

Some operators prefer the ferrous-oxalate developer, which is composed of oxalate of potash dissolved in warm water to saturation, and in this is also dissolved oxalate of iron (ferrous-oxalate) till the solution will take up no more. This solution may be purchased ready for use, if preferred.

The plate having been exposed, and everything necessary got ready, it may be placed, face upwards, in the dish, and sufficient of the developing solution may be poured into the dish, so as to completely cover it. After a few minutes, the negative may be examined, and when sufficient density appears to have been obtained, it may be removed from the dish, and the oxalate solution returned to the stock-bottle. The negative must now be rinsed with water, and then put into the solution of alum, which is made by dissolving alum in warm water until no more will dissolve. Of course, all solutions must be used *cold*.

Some gelatine plates have a tendency to *frill*. This is caused by the expansion of the gelatine film by the absorption of moisture. The effect of the alum solution is to harden the film. If the plate be touched with the finger while wet with the developing solution, it will be smooth to the touch; but after immersion in the alum for a minute or two, when touched again, it will be noticed that the character of the surface has changed, and this indicates that the film is sufficiently hardened. The plate may now be rinsed under the water-tap, and is ready for the next operation—fixing—and this is effected by immersion in a saturated solution of hyposulphite of soda (or thiosulphate of soda, to use the correct name).

This method of development has the advantage of being clean and very easy to manage, but it has the disadvantage that the solution is not constant, and the results are not always to be relied on. Of course, it is possible to become so familiar with a process that difficulties in the hands of one operator are not met with by another.

Development with pyrogallic acid is the method in most general use. Mr. Kennett's formula is:—Solution No. 1, pyrogallic acid 4 grains, water 1 oz. Solution 2, ammonia (sp. gr. ·880) half an oz., water 8 oz. Solution 3, potassium bromide 3 drs., water 8 oz. Solutions 2 and 3 may be kept mixed together. It is not necessary to weigh the pyrogallic acid for every plate. Keep a small bone spoon for the purpose, and after once weighing the proper quantity, notice what is required for the size of plate, and with the spoon sufficient may be *guessed*, a small quantity more or less being of very little consequence. But if Mr. Kennett's method be followed, take of No. 1 one ounce, and add to it one dram (or drachm) of the ammonia and bromide solution, which must be poured over the plate while lying flat in the tray. If any air-bubbles appear on the plate, they must be carefully removed, and the

solution must be kept in constant motion, not violently, or air-bubbles will be caused. In a short time the picture will appear, but the development must be continued until a strong image is seen when the plate is examined by transmitted light. Experience with a wet plate is of little use here, as the appearance of the image is quite different. The development must be continued until there is an appearance of great density, and the back of the plate being examined, the picture should to some extent be visible there. Practice with a few plates will be worth more than pages of written description.

Any make of gelatine plates may be developed with either of the solutions named, but in some cases it is advisable to follow the directions to be found with each packet of plates.

If the picture do not appear in moderate time, the exposure may have been insufficient, and more of the bromide and ammonia solution may be added. If the picture appear too quickly, it shows that the exposure has not been correctly timed. It is as well to add too little of the bromide solution, as it is easy to add more if the picture be tardy in developing. Under-exposure is apt to produce hard negatives and over-exposure too little density.

Too much attention cannot be given to the washing of gelatine plates. Running water may be used—a gentle stream is sufficient—or the plate may be placed face downwards (not touching the bottom of the dish) so that the thiosulphate of soda may be washed out, the water being changed several times.

Should the negative appear to be too thin, it may be intensified as follows: While wet, flow over the plate a saturated solution of bichloride of mercury. Notice the effect, and take care not to over-intensify. At the proper moment return to stock-bottle and wash the plate thoroughly; then flow over it a solution of ammonia, one ounce to eight ounces of water. This will darken the image, and the plate must be again thoroughly washed. The mercury and ammonia solutions need not be thrown away, as they can be used till exhausted. If the mercury (or *intensifying*) solution be found to act too vigorously, it may, of course, be weakened by the addition of water. Simply flowing the solution once over the plate is often sufficient, while in other cases the action of the bichloride may be allowed to continue until the picture is bleached white.

While wet, a gelatine plate must not be dried by a fire, as a collodion plate may be. It has been suggested that if surface-dried with blotting-paper, the plate may be dried by fire-heat. If carefully done, this method is effectual. When dry, the negative may be varnished in the usual way; or if only a few prints are required, the negative may be printed from without varnishing, care being taken not to use the paper damp.

## ON SOME PUBLICATIONS OF THE CHRISTIAN KNOWLEDGE SOCIETY.

THE Society for Promoting Christian Knowledge has now for many years been remarkable for the singularly able manner in which its Committee of General Literature and Education have conducted the publishing arrangements of the Society. No sectarian influences are allowed to interfere with the quality and character of the books published: in every case the question which the committee seems always to have had in view is whether a subject will be treated ably, honestly, and thoroughly.

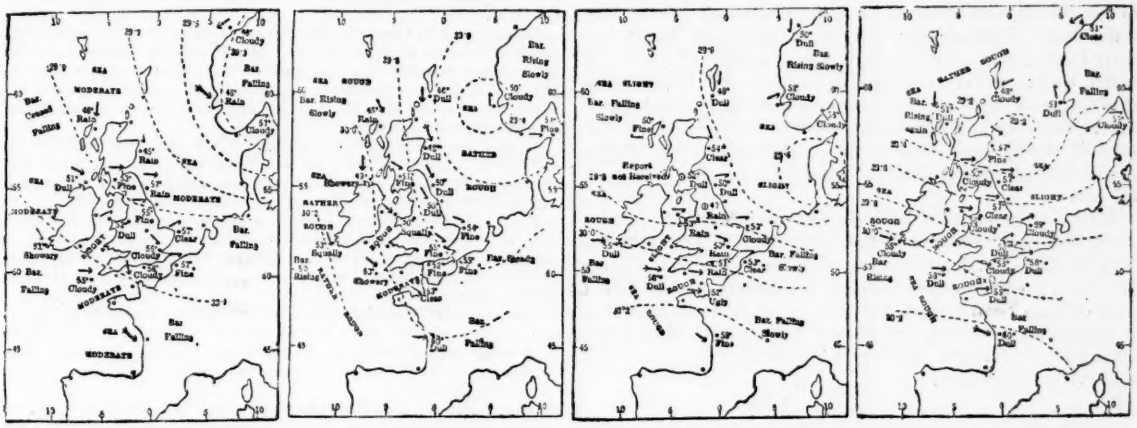
THE MANUALS OF ELEMENTARY SCIENCE,

for example, have been prepared, without exception, by writers thoroughly conversant with their various subjects. From Professor Fleeming Jenkin we have an admirable treatise on Electricity, clearly and brightly written—and we need hardly say, soundly. Mr. Le Gros Clark has written an excellent treatise on Physiology: one of the very best extant, for its size. Those who take interest in Crystallography will find the manual by Mr. H. P. Gurney full of useful matter, verging strongly towards the mathematical; but the nature of the subject rendered this almost unavoidable. The treatise on Matter and Motion, by the late Professor J. Clerk Maxwell, is open to more serious exception in this respect, having unquestionably gone beyond the line which separates the profound from the popular. Professor Clerk Maxwell was never able to appreciate the difficulties which beginners find in dynamical and kinematical matters; but in this little book he seems to have thought that the best way to avoid them was to pass boldly into the region where vectors freely roam, and the result is mystery and misery to beginners. It does not content them to learn that, "as indicating an operation, AB is called a Vector," and that "the operation is completely defined by the direction and distance of the transference"; nor is it any satisfaction to the inquiring mind to know that "the starting-point, which is called the Origin of the Vector, may be anywhere." Yet this is all which Professor Clerk Maxwell tells them about vectors in defining these useful articles. The S.P.C.K. should have a manual prepared, to which Professor Maxwell's might serve as a sort of sequel; for, unquestionably, a student of matter and motion is much aided, later in his studies, by the vectorial method. At present, however, it must be admitted that the manual by Professor Maxwell is an exception (and a solitary one) to the generally useful character of these science manuals. Mr. Bernays' manual on Chemistry and Mr. Newton's on Zoology are excellent. Of Mr. Proctor's manual on the Spectroscope we must not say more than that the author has honestly endeavoured to bring the work up, in clearness of exposition and fulness of matter, to the standard of the series. Astronomy, by Mr. W. M. Christie, now Astronomer Royal, is a useful and instructive little treatise, not absolutely free from errors (witness the singular inversion of the effect of longitude on time, in the diagram on page 14, where places west of Greenwich have their time marked later than Greenwich time, and places east of Greenwich have their time marked earlier!), but, in the main, sound. The illustration of orbits of comets at page 93, however, is misleading. Mr. Christie explains, indeed, how and why he has changed the shapes of certain cometic orbits; but he says nothing of the entire change he has effected in the positions of all the paths. It is on such erroneous teaching that the paradoxists erect their surprising structures. If there is one point in which books of astronomy more require re-modelling than any other, it is in this—the illustrations. The subject which, perhaps—of all treated of by science writers—most requires care in illustrating, is just the one which, as a rule, is most villainously illustrated.

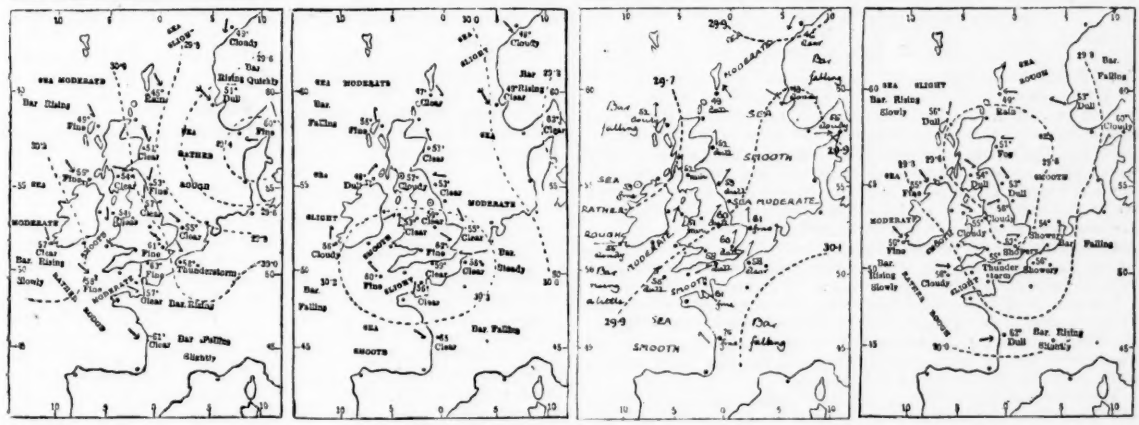
OVERHOUSE WIRES.—There are 9,000 miles of telegraph and telephone lines in the streets and on the house-tops of New York city; 3,500 of these belong to the Metropolitan Telephone Company. These figures should be a warning to our metropolitan authorities, for the number of telephone wires over London is increasing at an alarming rate. The Post-Office has spent an enormous sum during the past few years in converting the overhouse into underground lines.

## WEATHER CHARTS FOR WEEK ENDING SUNDAY, JUNE 18.

SUNDAY, 11TH. MONDAY, 12TH. TUESDAY, 13TH. WEDNESDAY, 14TH.



THURSDAY, 15TH. FRIDAY, 16TH. SATURDAY, 17TH. SUNDAY, 18TH.



In the above charts the dotted lines are "isobars," or lines of equal barometrical pressure, the values which they indicate being given in figures at the end, thus—30·4. The shade temperature is given in figures for several places on the coast, and the weather is recorded in words. The arrows fly with the wind, the force of which is shown by the number of barbs and feathers, thus:— ⟶, light; ⟶, fresh or strong; ⟶, a gale; ⟶, a violent gale; ⊙ signifies calm. The state of the sea is noted in capital letters. The * denotes the various stations. The hour for which each chart is drawn is 6 p.m.

## Reviews.

### "CURIOSITIES OF VEGETATION."*

IN this work Mr. Cooke coquets with the Darwinian theories. The book is interesting, and in parts instructive; but it is undeniably thin, and it wants plan and purpose. The author's object has been to summarise the results of the researches made by Darwin and others into the peculiarities of plant life, and to present their teachings in as succinct a manner as the subjects permit; but he makes scarcely any attempt at classification; nor does the reader, as he passes from chapter to chapter, recognise duly the scientific bearing of the evidence. The work reads like a note-book, rather than like a treatise on a scientific subject; and, even as a note-book, portions are scrappy and valueless. The book is more likely to be dipped into than read; and, viewing it in this way, we can recommend it as one which can hardly be opened anywhere without something of interest turning up. But if the book had been reduced to two-thirds of its present size (by the omission of such comparatively unscientific statements as that rattans are the terror of schoolboys, and that Krishna gambolled with the milkmaids of Brindabun, under the Kedamba tree), and its subject-matter duly arranged and systematised, it would have had greatly-increased value. Still, the book is one well worth reading.

* "Freaks and Marvels of Plant Life; or, Curiosities of Vegetation." By M. C. Cooke, M.A., LL.D. (Published by the Society for Promoting Christian Knowledge. London.)

### "STORY OF A MUSEUM."*

THIS is a capital book for young people, but also contains much excellent and amusing reading for older folk. The author shows what can be done in the matter of collecting objects of interest for a museum, when

* "The Story of our Museum; showing how we formed it and what it taught us." By the Rev. H. Housman. (Published by the Society for Promoting Christian Knowledge. London.)

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the task is set about in earnest. Mr. Housman expresses regret, and rightly, at the indifference to natural history and science too commonly manifested by the young men of the present day. "Brought up with their eyes unopened to the wonders of Nature, they pass through life in the same state of blindness; leisure often hangs heavy on their hands; the country, save for purposes of sport, is voted dull; travel is deprived of some of its greatest charms; and *wisdom at many entrances quite shut out.*" He notes truly how completely all this is reversed in the case of those who have imbibed an enthusiasm for studying and collecting, in any department of natural or historical science.

Every page of this book is full of interest, from the most amusing account of the author's first attempts at bird-stuffing (the illustration at p. 35 is worth the price of the book, to our mind) to the closing chapters on antiquities and coins. This is just the sort of book to select as a present for a clever, intelligent boy. Mr. Housman's remarks on Natural History Collections (Appendix A.) deserve careful study.

## BUTTERFLIES AND MOTHS.

BY W. J. H. CLARK.

WE have now enumerated the principal and most generally employed methods of catching, rearing, and preserving both butterflies and moths, but there are several others more or less efficacious which find favour in the eyes of different individuals.

On the Continent, especially in France and Italy, old soap-suds which have become putrid are considerably used, and they appear to attract moths fairly well. We have tried the plan ourselves, but up to the present only slight success has attended our efforts; however, we mean to give it a few more trials.

Bill-poster's paste, when stale, is very "fetching," and fruit which has burst through over-ripeness makes another very good bait, especially for butterflies and other day-flying Lepidoptera.

The flowers of the ivy and sallow are also very great attractions to moths, and have a peculiar intoxicating or stupefying effect on the insects, which, whilst under this influence, can be knocked off the flowers into a net without the slightest trouble. This method must be worked at night with a lantern.

Besides these, the following flowers, together with many others, are often visited by both diurnal and nocturnal Lepidoptera:—The heather, mistletoe, bramble, catchfly, viper's bugloss, valerian, several kinds of grasses, scabious, honeysuckle, &c. The flowers must be well watched, and when the insect is hovering over or has settled, the net will have to be used quickly, and with precision, or failure will result. Most of the hawk moths can be caught in this way, as none of them come freely to sugar.

The females of some moths also have an extraordinary attracting power for males of the same species, the "scent" appearing to travel incredible distances, and by exposing one of these virgin females in a small box covered with a piece of fine muslin, numbers of the other sex will come flying round in great excitement, and settle on the box, or near it, when they can be easily taken.

Probably the females of all moths have this peculiar power in a greater or less degree, but in the case of the Vapourer (*Orgyia Antiqua*), the Oak Eggar (*Bombyx Quercus*), the Emperor (*Saturnia Carpini*), the Kentish Glory (*Endromis Versicolor*), and one or two others, it is so very marked that even a person who knows nothing of entomology cannot fail to notice it.

The Purple Emperor (*Apatura Iris*), the most beautiful of our butterflies, and at the same time the most difficult to capture, possesses a very disgusting and depraved taste, the greatest delicacy one can offer him being decomposed animal matter, and if a dead horse, donkey, or other animal in a state of putrefaction happens to be placed near his haunts, he is sure to descend to the feast.

This noble butterfly seldom visits the ground unless under these circumstances, always flying about the tops of tall oak trees, hence the difficulty experienced in catching him. Some people use a net with a handle 30 to 40 feet long, but it is so exceedingly unwieldy and awkward that we cannot recommend its use; the only way we have found succeed being to wait beneath the tree until his majesty condescends to come to earth, when a sharp sweep with the net may capture him.

Light is very attractive to many moths; and if on a still warm night the windows are left open and a strong light kept burning in the room, numberless insects will flock in, apparently fascinated by the glare, and the operator, by standing near with a net, can capture as many as he requires. They must, if possible, be taken immediately on entering, as, if not, the wings are sure to become more or less injured, owing to the frantic efforts of the moths to get to the light.

Going round to the street lamps from about eleven to one o'clock at night is often very productive; and if half-a-sovereign or so be given to the man who extinguishes the lamps, and a supply of chip boxes, with instructions to catch and bring to you all the moths he finds, very many rarities will be obtained, and a considerable amount of trouble spared.

If any particular species is desired, the most likely spot for its capture is the tree or bush on which the caterpillar feeds, as the female moth, impregnated or otherwise, always flies about in its neighbourhood, and the males are generally not very far off.

All the above-mentioned methods are in use, and are accompanied with a greater or less share of success; but for moths, nothing comes up to "sugar" in attracting properties, as far as my experience goes, and I should advise collectors to stick to that and the street lamps.

Immediately on seeing Mr. Mattieu Williams' communication in No. 32, page 27, we gave the bisulphide of carbon a trial. The victim, a large Puss Moth (*Cerura Vinula*), was placed in the vapour as directed, for the space of five minutes, and when taken out was quite dead.

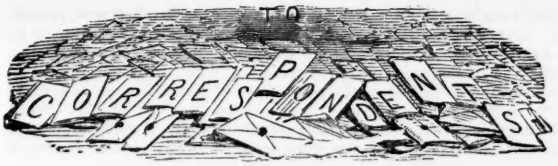
This method appears to us to be very good, as insensibility ensues almost immediately; and though death does not come on so soon as by some other means, yet the insect cannot flutter about, which is a great consideration.

There is one great obstacle attached to its use, that is the excessively unpleasant odour which it possesses; and, as the liquid is remarkably volatile, this property soon makes itself evident.

We thank Mr. Williams for the information, and for the future shall always keep some of the liquid in the house; we advise our readers to do the same.

TIME SIGNALLING.—The following information, relating to time signalling in England, is taken from the report of the Astronomer Royal for 1881:—"There has been only one case of accidental failure in the automatic drop of the Greenwich time-ball. On four days the ball was not raised, on account of the violence of the wind. The Deal ball has been dropped automatically at one hour on every day throughout the year, with the exception of fifteen days, on which there was either failure in the telegraphic connection or interruption from telegraphic signals continuing up to one hour, and of one day when the current was too weak to release the trigger without the attendant's assistance. On three days high wind made it imprudent to raise the ball. The Westminster clock has continued to perform well, its errors having been under one second on 40 per cent. of the days of observation; between one second and two seconds on 44 per cent.; between two seconds and three seconds on 14 per cent.; and between three seconds and four seconds on 2 per cent. Time signals originating in the Observatory are distributed at 10 a.m. to all parts of the country by the Post-Office telegraphs."

A PHENOMENAL CANARY.—The power of imitation possessed by birds of the parrot tribe has long been familiarly known, and it would not be difficult to find numerous examples of even well-educated members of the genus in this respect. We do not, however, usually regard the vocal powers of canaries as being equal to the production of articulate sounds resembling those made by the human voice. But there is at present in the possession of Dr. J. McGrigor Croft a little songster of this description, which, besides giving utterance to delicious warblings, is also able to "talk" with a clearness and precision simply marvellous. Somewhat sceptical of the accounts we had received of this animal wonder, we have, through the kindness of Dr. Croft, had an opportunity of directly proving the truth of the statements made concerning it. The canary does veritably *speak*, and enunciates a number of sentences which are clearly imitative of the voice of the lady who has had care of it since its early youth. The effect, indeed, produced by the clear, sweetly-uttered sentences pronounced by the bird is almost weird at first; but the feeling of wonder thus created quickly gives rise to a sensation of exquisite pleasure, which is deepened as the little creature suddenly at the end of a sentence rushes off into an ecstacy of song. As illustrating the exquisite pliability of the laryngeal apparatus of small birds, and the extent to which training may be carried in such cases, the tiny animal is deeply interesting to physiologists. As a mere curiosity, however, it is undoubtedly unique, and we are deeply obliged to Dr. Croft for having been enabled to witness so phenomenal a bird.—*Medical Press.*




# Letters to the Editor.

*[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]*

*All Editorial communications should be addressed to the Editor of* KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

*All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.*

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

*All Letters or Queries to the Editor which require attention in the current issue of* KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

## TOBACCO AND CONSUMPTION.

[431]—In mentioning the immunity of the Turks from consumption, Mr. W. B. Wicken (411) has not noticed the fact that the Turkish women have never "taper waists." If all the females of cattle and horses were deformed, would any sane person expect them to produce healthy or "sound" offspring? Now, fully 90 per cent. of the women here are so deformed. From the practice in youth of wearing stays and glove-tight dress bodies, their lung-capacity is reduced, and all sorts of morbid conditions follow; yet so strong is the force of habit, that the majority of the world expect to find the children of such mothers healthy and unpredisposed to consumption, and the scientific even cast about to try and discover some far-off and occult reason for the most natural result! The marvel is that the nation is not more unhealthy. Rather let them use their influence to try and persuade young men and women to study anatomy, and to go and look at the statues in art galleries, and instead of calling a woman's natural figure "clumsy," to fix that epithet on a dress which makes it appear so.

They will see there, also, that legs are no more exclusively masculine than arms or fingers, and they may in time begin to draw the inference that a style of clothing which, from its innate ugliness requires the sacrifice of health and strength to make it presentable, is not the most advisable to persevere in upholding, if they wish to preserve the supremacy of their race, and to stamp out lung disease.—I am, Sir, &c., F. W. HARBERTON.

[432]—There are no returns showing the relative prevalence of consumption among the smoking and non-smoking males of this country, but some idea of the influence of tobacco may be obtained by comparing the deaths from phthisis of males (of whom, perhaps, one-half are smokers) and females, who are, practically, all non-smokers. The deaths from phthisis in the year 1876, for example, in England and Wales were 36,443 males and 35,221 females. The occupation of men is the chief cause of the greater prevalence of phthisis among them, but, if tobacco had any practical power of averting it, we ought certainly to find its frequency less among men than among women. J. J. RIDGE, M.D., B.A., London.

## MULTIPLICATION AND DIVISION OF DECIMALS.

[433]—With your permission I venture to submit the following facts relating to this subject to your arithmetical readers, with an apology if it happens that I am telling them anything that they already know.

Arithmetic is an *exact science*, and, therefore, does not entertain the idea that when applied to calculations based upon measurements, it is working upon a more or less erroneous foundation. As a matter of fact, however, all measurements are necessarily approximations. If we reckon in the decimal notation, our measurements will be correct to so many decimal places, and no more. Suppose then that we require to multiply the numbers 3612·249 and 3·141, both of which being correct to the third decimal place, the error in each factor (due to the fourth, fifth, &c., decimal places being unknown) will cause corresponding errors in the product; but the fact is evident, that the error in the second factor (3·141) will cause a thousand times more mischief than the error in the first factor, for it is multiplied by a number a thousand times larger than is the case with the other. Is it not wise, therefore, to cut down the first factor to the same number of digits as the second, and multiply 3,612 by 3,141?

Again, since the product of two approximate numbers can never be depended upon to more digits counting from left to right than there are correct digits in *either* factor, will it not be wise to substitute cyphers for all digits beyond four in the product of the above numbers, and write 11340, instead of 11345·292? This makes our results appear to be often only very rough approximations; but what is the use of deceiving ourselves? I have seen the heating surface of a set of boilers calculated in square feet, and the result published to *two decimal places*, when a moment's consideration would show that an error of only one-hundredth of an inch in the diameter of the tubes (of which the heating surface mainly consists) would vitiate the result to the extent of twenty or thirty square feet.

Logarithms appear to have this advantage over ordinary arithmetic, namely, that they naturally fix the degree of accuracy as well as the value of our results. Logarithms put us to a lot of trouble if we attempt to get more figures in our answer than are good for us. —Yours, &c. A. N. SOMERSCALES.

[434]—Supposing that the reader has learnt how to reduce a vulgar fraction to decimal form, *i.e.*, how to divide one integral number by another not contained "exactly" in it, and to carry on that division beyond integral quotient figures, and that he accepts the truth of the statement, that "if the divisor and dividend be both multiplied by any the same number, the value of the quotient is unchanged;" then the rule for division of decimals is, "multiply the divisor and dividend by such a power of 10 as will remove all decimals, and divide the latter product by the former."

I take the examples given in KNOWLEDGE, May 6, p. 576:—

```
127·412)  15·315
   1000      1000
  ------    ------

or, 127,412)  15315·0(·1202006 . . . . . .
              127412
              ------
               257380
               254824
               ------
                255600
                254824
                ------
                  776000
                  764472
                  ------
                   11528

127·412)  1531·5
   1000      1000
  ------    ------

or, 127412·)  1531500·(12·02 . . . . . .
              127412
              ------
               257380
               254824
               ------
                255600
                254824
                ------
                  776
```

From the above the following rule at once suggests itself:—

1. If the number of decimal places are not equal, make them equal by annexing zeros.

2. Then divide as in whole numbers, neglecting the decimal points in division and dividend. *So far*, the quotient will be integral.

3. Any quotient figures found by annexing zeros to the dividend, or remainder, beyond this point, will be decimals, but the only re-

commendation it has is that it saves the trouble of writing the figures down a second time. W. MATHIAS.

The School, Bideford.

P.S.—You will observe that I reverse the usual order of things. In all arithmetic text-books that I am acquainted with, division of decimals *precedes* the rule for reducing a vulgar fraction to a decimal, whereas division of decimals might aptly and naturally follow long division.—W. M.

## METEOR.

[435]—I see, in answer to a correspondent "Euclid," of Bolton, (KNOWLEDGE, No. 31, p. 13), a reference to a large fire-ball, or meteor, seen on May 4th last, at 9.30 p.m. I saw it, and as you indicate that more information on the subject might be useful, I venture to give you my version of it from notes made at the time. I was searching with my telescope for $\psi$ Cygni, when suddenly the field of view was lit up, and the stars in the field vanished as if daylight had suddenly returned. On taking my eye away to ascertain the cause, there, right in front of me, was a great ball of intense white light, apparently almost as large as the full moon, gliding across the sky. It travelled so quickly that it is almost impossible to name its exact path, but when I first saw it it was about on the head of Draco. It passed over Cepheus and into Cassiopeia. In passing over Cepheus its colour changed from white to red, and near $\gamma$ Cassiopeiæ, it again changed to blue, or violet, gave an extra flash up and instantly vanished, leaving behind it a long train of red sparks, which died out by degrees, and which had previously been invisible in the intensity of its light. The whole phenomenon did not occupy more than two or three seconds of time, and there was no perceptible noise. It lit up the whole sky like a lingering flash of lightning. It certainly was an awe-inspiring sight, and I heard many people in the neighbourhood, when it was taking place, give vent to their feelings in a shout of amazement. I never saw anything approaching it for grandeur in my life.

EXCELSIOR.

[For the purpose we mentioned, "that is, to determine the true path of the meteor in our atmosphere," it would be essential that the exact course of the meteor, as seen from our correspondent's station, should be indicated, but his letter is independently interesting.—ED.]

## FLINT INSTRUMENTS AND PRE-HISTORIC MAN.

[436]—A friend of mine, who has just returned from Natal, and who had heard nothing of the discussions and theories on the above subject, and on whose veracity I can thoroughly rely, has been employed in making the Durban and Maritzburg Railway. In conversation the other evening, he told me that in making the Umgeni cutting (through red loam, gravel, and limestone rock) at 14 feet from the surface, from which a dense forest had been previously cleared, in the red loam his Kaffirs came upon the remains of a fire, charred sticks, &c.; close by this he found what he describes as a well-made and beautifully-finished flint adze-head, the cutting-face sloping from one corner to the other, with a bevelled edge like a chisel, and the other end finished off with a round flat knob.

Again, at 40 ft. from the surface, in the hard gravel, he found a *good many* flint instruments, the two most remarkable ones being a round stone, about the size of a large orange, very much flattened at each pole, with a ¾ in. hole drilled through it, and by the side of it a stone handle 7 or 8 in. long, one end just fitting through the hole, and the other end rounded off; when put together it had just the appearance of a small stonemason's mallet. At a short distance from this was a stone quoit, almost exactly like the iron ones at present in use, except that, from where the indentation for the thumb is, the circle was cut straight across, for about quarter of the circumference, by a round handle. The whole of these curiosities were taken possession of by the engineer of the line.

When the geological formation of Natal, and the pretty well-founded idea that the whole of the country has been denuded by a great out-rush of waters from a great lake once covering nearly the whole of what is now the Orange Free State, is considered, we are compelled to give an antiquity to the possessors of these implements as great as, or even greater than, that given to similar tribes of men in Europe. W. M.

## THE POTATO.

[437]—"Farmer" (405, p. 611) asks me whether I advocate the use of mineral manures to increase the starch in potatoes. I take it for granted that by the term "mineral" is meant potash, salts, &c., without reference to superphosphates. My own experience (not at second-hand) indicates that the percentage of starch, as found under the influence of "supers," is reduced by potash salts; but the land on which I operated was naturally well supplied with potash, so that only a guess can be made at the probable results to be obtained on land poor in that element. Märcker finds but little effect on the starch from any manure. I can give no information concerning the effect on the percentage of starch in cereals exerted by "minerals." "Farmer" is somewhat hard on "F.C.S." when he says that it is nonsense that stable manure is not better than peat; therefore I add for comparison the analyses of a sample of peat and of farmyard manure:—

| | Farmyard Manure. Per cent. | | Peat. Per cent. |
|---|---|---|---|
| Organic matter | 78·8 | ..... | 77.0 |
| Mineral | 3·3 | ..... | 3·0 |
| Potash | 0·15 | ..... | 0·07 |
| Sulphuric acid | 0·11 | ..... | 0·26 |
| Lime | 0·2 | ..... | 0·25 |
| Nitrogen | 0·58 | ..... | 0·99 |

Another sample of peat contained less nitrogen, but nearly double the amount of potash. Chemically speaking, this peat was in many respects better than manure, but the agriculturist would probably not consider it so, because he would find that the fibre of the peat would take so much longer to rot; also the nitrogen of the farmyard manure is in a more assimilable condition than that of the peat. It is much to be regretted that the scientist so frequently makes a statement which, though correct in itself, requires some further explanation before it becomes intelligible to the public, who, being unaware of the full facts, proceed to judge upon the matter. Because I fancy that this has occurred in the present instance, I have added this short explanation, which will, I trust, quiet "Farmer's" mind on the subject of peat, and also show that F.C.S[t] was correct in what he wrote—although, I daresay, he is perfectly well able to take care of himself, without my saying anything.

E. W. P.

## A CALCULATION.

[438]—A solution to the following question would oblige one of your contributors. A Building Society is formed, consisting of 500 members, for the purpose of advancing a sum of £500 to each member for a term of 12½ years, free of interest. Subscription of each member £12 per year, repayment of loan at the rate of £40 per year: how long will it be before the final member receive his advance? PYRAMUS.

Liverpool, May 23, 1882.

## GOLD IN INDIA.

[439]—Much has lately been written anent the, at present, disputed existence in India of quartz that will, on proper treatment, yield gold in quantity. Will one of your scientific friends who knows the regions there at present under exploration, give me, through your medium, his opinion on the subject? This question interests many, if not all, of your readers.—Yours faithfully,

J. T.

## INSTINCT AND REASON.

[440]—May not instinct in the lower animals and reason in man be the same, with this difference—that the one is more intense in character, the other wider in range? And may not this difference be accounted for by the fact that, while the brute does one thing a thousand times, we do a thousand things once? SILCHESTER.

## "AULD ROBIN GRAY."

[441]—Concluding that the subject of music lies within the scope of KNOWLEDGE, may I hope that you will throw some light on the dispute touching the authorship of the melody associated with the words of "Auld Robin Gray?" The words are the undisputed production of Lady Ann Barnard. The melody, claimed in 1812 by the Rev. W. Leeves as his own composition, was familiar to my mother in her early youth, she having been born in 1784. Both melody and words have always been accepted by myself and many others as the work of one and the same mind. It would be a worthy tribute to the memory of Lady Ann Barnard to prove that the exquisite melody was hers, as there seems internal evidence that it really was. E. L. H.

NEXT week the series on "How to Get Strong," "Home Cures for Poisons," and "Electrical Generators," will be continued. A valuable paper on "How to Ride on the Tricycle," by Mr. John Browning, treasurer of the London Tricycle Club, will also appear. An important series of articles on Geology, by Mr. W. Jerome Harrison, F.G.S., will be commenced shortly, and several important papers by Messrs. Prof. Charles Tomlinson, Grant Allen, Edward Clodd, W. Mattieu Williams, Dr. Wilson, and others are in type.

# Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 4. *Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

PECCAVI. I had *not* been able to read the question relating to the British Museum, or I should not have failed to thank you warmly for your very kind offer, but at present I have no occasion to avail myself of your kindness. With regard to the beam question, you must remember what the light has to do to make the optic nerves sensible of its source. A wave may exist when it reaches the shore, yet be too small to disturb the sand there.—J. CRAM. Thanks for magic cube on base of 11. Fear it may be some time before we find a vacant corner large enough for its insertion.—TARANAKI. You need no forgiveness "for being too plain spoken." Our waste-paper basket is n t very ready to take offence.—PARSON. Thanks for kind and encouraging letter. We agree with you that the mere dilution of science into amusing twaddle would not be an improvement to KNOWLEDGE. We certainly do not intend to become popular in that way.—THOUGHTFUL. Yes; pamphlet safe.—SILCHESTER. Fear we could not find space for any more articles on health resorts than we have already provided for.—F. W. FOSTER. Is there anything to show that the vapour is other than a light mist, the condensed vapour of water?—T. J. HICKIN. I do not think that a copy of Sir Isaac Newton's "Opticks" (third edition) has any special pecuniary value.—H. R. WELLER. We cannot understand your difficulty. It is proved that under the attraction of the sun, a body, in whatever course it may be moving, will travel in one of the conic sections, according to its velocity at any given distance. The particular conic section described will be an ellipse or a parabola, or a hyperbola. The planets have such particular velocities as correspond with motion in an ellipse.—J. W. JAQUES. Attempting to indicate the distance between two heavenly bodies or the apparent size of one, in inches, feet, yards, furlongs, &c., is, as you say, utterly absurd. One might as well indicate the distance between two places in hogsheads and kilderkins.—H. D. The idea that there is life on the farther side of the moon is an old one, but is quite untenable. We have not yet seen it accompanied by the idea that on the side turned from us the moon may have an appendage like a comet's tail. You are mistaken about the centrifugal tendencies existing on the moon. They are such solely as result from her rotation on her axis once in $27^1$ days. The only cause to be considered as possibly tending to produce a difference in the condition of the moon's invisible hemisphere is the earth's attraction. This, however, can produce no such effects as you imagine. The whole subject, however, has been so often discussed that it is now overworn.—J. M. FISHER. We have not your letter. We did not insert it, because it promised to initiate a controversy, and we have no room for controversy. Dr. Wilson's answer was intended for you alone; and the method we adopted was the most space-saving one we could employ. We do not know of any rule of "fairness" requiring us to insert every letter of the kind which may be sent to us. We know that fairness to the proprietors utterly forbids our doing so. If, however, you still think Dr. Wilson mistaken, and will succinctly say in what points, we will insert your remarks.—SUNDEW. (1.) Are you sure it was the zodiacal which you saw on the 7th soon after ten? June is not usually a good time to see the zodiacal; note the position of ecliptic in our June maps. (2.) The rainbow is not necessarily part always of the same-sized circle. It can have two angular diameters, and the apparent size, even for the same angular diameter, varies according to the distance of the cloud on which the rainbow is seen. —AN OXFORD UNDERGRADUATE. Those papers are rather letters than articles; however, there will be no more of them.—M. B. ALDER. It must be satisfactory to know so certainly about the moon. I wish astronomers did. Your remarks about warm weather some time ago read strangely now, after so much unseasonable cold.—A.B. Solution correct, but author's (shorter) has appeared.—A STUDENT. We should in that case see the moon turn slowly on her axis (apparently backwards) in such sort that every part of her surface would in turn become visible to us.—ASMODEUS. It is hardly in our line to give rules for preventing the beard from growing.—E. M. The number of stations at present existing is far too small to give even the roughest approximation to annual rainfall for whole earth. The sun-spot theory of rainfall is not believed in. It was only started, I fancy, to get a pull on national purse-strings.—G. T. M. E. Thanks.—H. E. KILBY. Yes; that method, like the others, is theoretically sound, but practically unavailable, because of the relative smallness of the moon's distance. Very small errors in determination of longitude and latitude of stations, and of times of contact, &c., would introduce very large errors into the estimates of the sun's distance. The conception is, however, very ingenious.—ERIN-GO-BRAGH. It is the case sometimes; so also old hens are adorned occasionally with imperfect sort of comb, and some old ladies with moustachios and beards.—A. BUTILE. The Leprosy communication did not seem quite so generally interesting and important as that on Tubercle, and our space ran short.—W. GRANDY. Thanks for encouraging letter. On the other point, I can assure you I would gladly see my books sold at very low prices; but publishers, with whom the matter rests, cannot venture—yet, at any rate.—A. E. W. Our arrangements for indexing already made when your letter received.—F. MORRIS. Berry's Polishing Paste and Domestic Black-Lead are rather outside our lines.—T. GREEN. Do not know of any book or books on Gum, its properties and uses.

## BOTANICAL.

E. C. H. (Worthing). Most cultivated "geraniums" belong really to the genus *Pelargonium*, which has slightly irregular flowers (two upper petals differing from three lower), a spurred sepal (scarcely noticeable), no glands, and few stamens (generally five). The true *Geraniums* have regular flowers, no spurs, five glands on the disk, and ten stamens. A few perennial true *Geraniums* from southern Europe are still cultivated in old-fashioned gardens, but most of the plants so-called are *Pelargoniums* from the Cape of Good Hope. *Pelargonium* is, in fact, a larger and more specialised insect-attracting type. When in doubt, look for the irregular sepal and count the stamens. GRANT ALLEN.

## ELECTRICAL.

IGNORAMUS. See article in KNOWLEDGE, No. 30; ebonite can be procured from the India-rubber and Gutta-percha Company, 100, Cannon-street, London, E.C. The same article (see also errata, No. 31) answers your query anent length of wire. The generator described will make a current quite strong enough for plating—in fact, rather too strong in proportion to its other features. It will not be strong enough for a Swan lamp, which requires an electromotive force of forty volts, or the force produced by twenty good Bunsen cells joined up in series.—H. BARDSLEY. Get a glass or earthenware jar, about three pints capacity, into this put a zinc rod for the positive element. The negative element is a small carbon plate placed in a porous pot, filled up with a mixture in equal parts of crushed carbon and black oxide of manganese, about the size of parched-peas. Great care is requisite in making this part of the cell, and you would find it cheaper to buy—well, the porous pot with carbon plate *in situ*. A saturated solution of sal ammoniac is put in the outer jar, half filling it. At Silvertown, where these batteries are made in very large quantities, the upper extremities of the carbons are dipped, at a high temperature, into melted paraffin, and subsequently placed into a mould containing molten lead, which forms a cap for the carbon, a brass terminal being simultaneously fixed into the lead. Marine glue is run over the carbon-manganese mixture, the carbon rod being in position. Vent-holes are made in the glue to allow any gases to escape. All bare metal connections must be well coated with pitch or varnish, otherwise they will soon be corroded by the gases given off by the battery. Should you buy a cell, I would recommend the new agglomerate Leclanché, with a large zinc cylinder, instead of the zinc rod. There are at the present moment some thousands of this latter form in use, comparing very favourably with the Fuller or double liquid bichromate battery.—CHARLES BUSBY. You have been anticipated long since. Inventors have not yet been able to overcome the apparent partial volatilisation of the carbon, as is evidenced by the thin carbonaceous film deposited on the surface of the glass globe.—IGNORAMUS. 1. Yes, the same. 2. See articles on "Generators." 3. No, not on an extensive scale. 4. The reversibility of the dynamo can be easily explained. It is due to a very simple truism, viz., the convertibility of forces. This point, we hope soon to be able to deal with under the head of "Electric Motors."

ACCORDING to the *Railway Age* (Chicago, April 20) the American Bell Telephone Company has in operation 189,374 instruments and 49,168 miles of wire. These are connected with 592 exchanges. The figures show a great increase on the preceding year. If the company is able to organise a satisfactory system of underground wires, a great further development may be expected. The charges will in this case be considerably reduced.

XUM

# Our Mathematical Column.

## THE LAWS OF PROBABILITY (*Concluded*).

IT should be noticed, in the first place, that in dealing with hypotheses, we must consider their antecedent probability as well as the evidence derived from observed facts. We sometimes hear the contrary course spoken of as the perfection of philosophic reasoning. "I will not suffer myself," some will say, "to consider whether this or that hypothesis is likely or not; I only desire to know whether observation and experiment appear to favour it or the reverse." This is an exaggeration of a principle which is excellent in itself. Doubtless observation and experiment should be the test by which every hypothesis is examined; and in the long-run, observation and experiment may serve to over-balance any antecedent probability against or in favour of an hypothesis. But during the progress of research, and while as yet the evidence derived from observation or experiment is not decisive, it is essential to the formation of the most probable opinion that antecedent probabilities should be duly weighed. For example, let us suppose that a person submits to my notice three predictions concerning the weather, founded on the planetary positions, and fulfilled in a very striking manner by the event. Let us suppose that the predictions are couched in the clearest possible terms, and are such that the odds must be regarded as heavy against their *accidental* fulfilment—say, 9 to 1 for instance. Then the chance that all these predictions will be fulfilled, if the hypothesis of planetary influence is erroneous, is $\frac{1}{1000}$; or the same as that of drawing one particular ball out of a bag containing a thousand. And the fact that the predictions have actually been fulfilled seems, therefore, to afford very striking evidence in favour of the hypothesis that the planetary positions do actually influence the weather. If I overlooked the antecedent probabilities of the hypotheses (1, that the weather is so influenced; and 2, that it is not), I should calculate the probability of the planetary hypothesis at no less than $\frac{1000}{1001}$, or very nearly unity—*i.e.*, certainty. But this would be to forget the main portion of the evidence, that, namely, on which our opinion of the antecedent probability against the planetary hypothesis is based. I do not enter into consideration of this evidence, but undoubtedly it is of such weight that the most surprising series of fulfilled predictions (even though accompanied by clear evidence that not a single prediction had been unfulfilled) would scarcely justify us in even giving the hypothesis consideration.*

It is necessary also to consider most carefully whether we really have before us all the possible hypotheses by which observed facts may be explained, and whether the various hypotheses are mutually exclusive, for otherwise the general theory above enunciated is not applicable. As an instance, let us consider the views of Dr. Stark respecting the influence of marriage on the death-rate. From the observed mortality among married and unmarried men (the former class including widowers), Dr. Stark was led to infer that marriage is favourable to longevity. (The statistics are given in my essay on the subject in "Light Science"). In arriving at this conclusion, he took only into account the particular hypothesis that the observed inferior value of unmarried lives is due to bachelorhood. The hypothesis that marriage is favourable to longevity is not unlikely in itself, though, certainly, the very marked difference considered by Dr. Stark seems more than the hypothesis can account for. But there is another hypothesis of which Dr. Stark took no account whatever: the superior value of married "lives" might be due to a principle of selection, whereby on the average of a great number of persons, the healthier and stronger would be found in larger proportion among the ranks of the married than of the unmarried. The neglect of this hypothesis vitiates Dr. Stark's reasoning altogether. The consideration of this hypothesis, to the neglect of the other, would be also incorrect. Both hypotheses must be considered together, with due reference to the antecedent probability in favour of each. But as they are not mutually exclusive, we cannot apply to them our general rule. We can only infer that either one or the other is the true hypothesis, or else that both hypotheses are partially true; and this does not leave us three cases only to consider, because the causes dealt with by the hypotheses may co-exist in different proportions. The statistics dealt with by Dr. Stark are, therefore, not capable of leading us to any definite conclusion.

But it is not to be inferred that in this case, and in other similar cases, the statistics are valueless. The fact that they point to no definite conclusion shows only that they are insufficient. Other statistical information may so supplement such imperfect knowledge as to lead to the definite conclusions we require. And the consideration of imperfect statistics usually shows us in what direction we are to look for supplementary facts. For example, to return to Dr. Stark's statistics. We have seen that they are insufficient in themselves. They point, however, to two hypotheses, and we have only to determine in what degree the two causes dealt with by these hypotheses actually operate, to form a sound opinion on the whole subject. I am not concerned, be it noted, with the practical application of such inquiries, only with the bearing which they would have on the special question considered by Dr. Stark. Now, if we study the statistics relating to the number of marriages in times of prosperity or adversity, or, again, in healthy and unhealthy seasons, we should gain some slight insight into the probability of the action of a principle resembling selection. We might obtain statistics, again, as to the trades of those who marry, so as to compare the number of married and single in the different trades. Other statistical inquiries (which might be very difficult to carry out in practice, however), such as inquiries relating to the insurance of married and single, to the health and strength of each person throughout his life, and so on, would throw further light on the subject of inquiry. Under the actual circumstances, we can only form probable inferences on these subjects, but the conclusion we arrive at is much more likely to be sound when such inferences are considered, than when no attention whatever is paid to one of the possible causes of a result so interesting as the superior longevity of the married.

Another circumstance to be very carefully considered is the actual number of cases with which our statistics may deal. It is impossible to arrive at correct conclusions unless this number is sufficiently large. Quètelet has related how a person assured him that a certain street in Paris was the healthiest region, not only in Paris, but in France. When Quètelet asked how this extraordinary conclusion had been arrived at, his informant remarked that Quètelet's own method of statistical research had been applied: "I have found that more than two hundred persons live in that street, and there have been only three funerals in three years. Now, in what part of France is the death-rate so low as five per thousand per annum?" "I suppose, then," answered Quètelet, "that if there had been no funerals at all, you would have concluded that by removing to that street you would live for ever."

One point remains to be touched upon. I have spoken of the fact that in any very great number of trials, series of events may be *expected* to occur which would appear very surprising if they happened in but a few trials. It would be a mistake, however, to infer that the occurrence of a strange series of events is to be regarded as on that account less worthy of notice. Boswell related that when he mentioned to Dr. Johnson that the first of the new hackney-coaches which he had seen had been actually No. 1, the Doctor ridiculed the notion that there was anything remarkable in the coincidence. "You were as likely," he reasoned, "to see that coach as any other." Now, although Dr. Johnson was certainly right in this instance, yet it would be a mistake to conclude that such coincidences in general should be overlooked.* When a coin is tossed, it is as likely that twenty heads will be tossed in succession as that any other specified arrangement will appear; yet undoubtedly if the event had happened we should be justified in suspecting that a two-headed coin had been used. If a die is cast twenty times, and ace appears eight times, we might reason that the observed succession of throws was antecedently as likely as any other particular succession, and this reasoning would be just; but, unquestionably, if such an undue number of aces appeared, we should think it not unlikely that the die was cogged. This opinion would be strengthened if ace was a

* Such an assertion as this seems incomprehensible to the paradox mongers (as De Morgan used to call those who propound such theories as we are considering). "Strike, but hear," they say, and they wonder that none will hear, and that few even think it worth while to strike.

* Quètelet relates a curious story illustrating this: "During the first Spanish war, a corps of the French army, which was employed in the siege of a town, dreaded the recurrence of Friday, as a day fatal to it, because the enemy killed or wounded then more of their men than on any other day of the week; a strong prejudice was, in consequence, established against Friday. Now after the siege, it appeared that the artillery had been changed every day, and that those who did duty on Friday were superior marksmen to the others. Superstitious ideas," adds Quètelet, "have often less foundation; from the impossibility of discovering the true cause under the circumstances in which we are placed, we are apt to attribute them to objects totally extraneous, not being willing to consider effects as existing without a cause."

throw by which money was won. Without deciding at once that we were being cheated, we should watch somewhat carefully to see in what proportion aces appeared thereafter. So it is when, at card games, a player secures more frequently than probability warrants some favourable "hand." There *must* be players who seem lucky on occasion, and there *must* be some who appear exceptionally lucky for years. But this should not cause the player to blind himself to the antecedent improbability of definite runs of luck, and therefore he should not overlook the necessity for watchfulness when they have occurred. And lastly, in those affairs of ordinary life in which chance plays a more or less prominent part, but conduct yet counts for something, we are justified in inferring skilful conduct where good fortune is long continued and strongly marked. "In the conduct of life," says the great mathematician Laplace, "good fortune is a proof of skill which should induce us to prefer placing our confidence in those who appear thus lucky."

## Our Chess Column.

By Mephisto.

### THE VIENNA INTERNATIONAL TOURNAMENT.

THIS week will see the finish of the Tournament, as on Wednesday, probably, the last game will have been played, and then the Chess world may devote itself to the task of honouring the victor. Who this would be formed the subject of conjecture and conversation in London Chess circles. Steinitz, who had been doing badly in the beginning, gradually crept to the front, till on the twenty-sixth round he came out ahead of all other competitors. The public like to pin their faith to tried men, and it was generally then taken for granted that Steinitz would come out as chief winner. This belief was, however, dispelled by the news that arrived from Vienna, which showed that Steinitz had relapsed into bad luck, while Mason was doing wonderfully well and carrying everything before him. According to telegraphic information received in London on Thursday, Mason was at the head of the list with 20, while Steinitz stood at 19½. It was then generally believed that Mason would win first prize, and bets were freely made on that expectation.

According to a telegram in the *Daily Telegraph*, of Monday the following was the state of the score on Saturday night, June 17, after the conclusion of the 31st round:—Mackenzie, Steinitz, and Winawer, 21½; Mason, 21; Zukertort, 20½; Blackburne, 18½; Englisch, 17½; Paulsen, 16½; Wittek, 16; Hruby, 14½; Weiss, 14; Schwarz, 13½; Bird and Tschigorin, 12; Meitner, 11½; Ware, 10. The committee have thought fit to alter the order of the play on Thursday last; we cannot therefore properly estimate Mason's score, as it is quite possible that in spite of his being half a game behind, his position might be as good as that of any other man. The closeness of the five first men makes the finish a very exciting one.

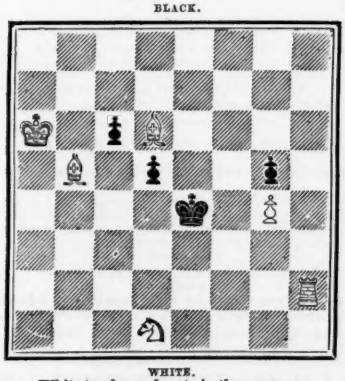
On Monday Zukertort beat Weiss, Paulsen beat Ware, Hruby beat Mackenzie, Steinitz *v.* Bird drew, Mason was engaged with Englisch, and Winawer with Meitner.

The result of the match, which concluded on Wednesday, is as follows:—Steinitz and Winawer each won 24 games; Mason, 23; Mackenzie and Zukertort, 22½; Blackburne, 21½. Steinitz and Winawer having tied, will have to play another game to decide which takes the first prize and which the second, and Mackenzie and Zukertort will likewise have to play a final game, to decide which takes the fourth and which the fifth prize.

Problem No. 45.

By B. G. Laws.

BLACK.



WHITE.

White to play and mate in three moves.

### SOLUTIONS.

Problem No. 42 by J. A. Miles, p. 615.

1. R takes P (ch) — 1. K to B5
2. Q to K6 — 2. R takes Q
3. Kt takes R (mate)

or if 2. K to Kt4

3. Q to R6 (mate)

or 2. R takes R

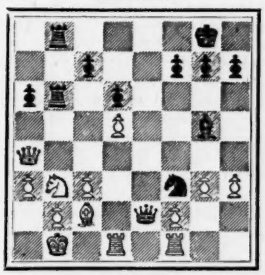
3. Q takes R (mate.)

By J. Licence, p. 16.

| No. 43. | | No. 44. | |
|---|---|---|---|
| 1. Kt to K4 | K to Q4 | 1. P to K8 B | K to K3 |
| 2. P to Kt8 R | K to K4 | 2. R to B6 (mate) | |
| 3. R to Kt5 (mate) | | | |

The following two pretty endings are from the *Field*:—

BLACK.
Blackburne.



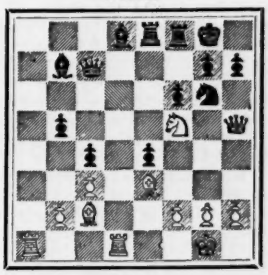
WHITE.
Winawer.

The game proceeded thus:—

| WHITE. | BLACK. |
|---|---|
| | R takes Kt |
| B takes R | Kt to Q7 (ch) |
| R takes Kt | B takes R |
| R to Qsq | B takes P |
| B to B2 | R takes P (ch) |
| K to Bsq | R to Ktsq |

Q to K4; and here Blackburne made one of his magnificent *coups*. He answered R to Kt8 (ch). Of course, the K was bound to take, and then followed Q to QKt4 (ch), and White's Q was lost. The game broke down soon afterwards, and Winawer resigned on the 52nd move, after 5½ hours' play.

BLACK.
Steinitz.



WHITE.
Blackburne.

In the position given in the diagram, White played Q to R3. Steinitz completely overlooked the ingenious device, and answered R to K4, whereupon Blackburne won the Q, by R to Q7. Of course, Steinitz was bound to take, for mate was threatened by R takes KtP (ch), and the black Q was therefore lost by Kt to R6 (ch). The proper answer for Black in the position given was Kt to K4, with the view of fixing himself at Q6, even if White attacked the R by Kt to Q6. Black would have had a fair game then at the cost of the exchange for a pawn.

### ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

W. L. Lambert *v.* Edward Wilson.

Novice.—We cannot find any fault with our score of game p. 633. There is no Queen's Pawn to take White's Knight or Bishop, as the Queen's Pawn has been captured by White "en pass" on his third move. Move 16, p. 634, it ought to be Q takes Kt.

Correct solutions of Problem received: No. 42, by Berrow; Nos. 43 and 44, by C. S. Bright, Genoa, Brenton, J. Russell (something).

S. M.—It is not so.

J. Tilbe.—To castle after check has been given, is allowed.

A report (just issued, but dated Oct. 21, 1881) on the incandescent lamps exhibited at the Paris Exhibition (1881), has been presented by Professor W. Crookes and G. F. Barker, who were appointed a sub-committee for the purpose. The tests have been carried out in an exhaustive manner, and it will be matter for very great regret if a similar report is not issued on the more recent exhibition, as it is certain that great improvements have been recently made, more particularly in the Swan and Lane-Fox lamps.

The case of "the United Telephone Company (Limited) *versus* Harrison, Cox-Walker, & Co." has terminated almost entirely in favour of the United Company, who sustain their patents for the Gower-Bell receiver, but are defeated in the matter of the Edison transmittor. They intend to bring the latter question before the Court of Appeal, and if they gain the day, they will hold almost exclusive sway in matters telephonic. Professor Sylvanus Thompson, writing to the *Times*, asserts, however, that the Dolbear system, so ably described by Professor Dolbear himself before the Society of Telegraph Engineers, a few weeks since, is unaffected by the decision. The principles differ considerably from those of the United Company's apparatus.

XUM

# Our Whist Column.

By "Five of Clubs."

PLAY THIRD IN HAND, PLAIN SUITS (*continued*).

WHEN, third in hand, you hold Queen or Knave and others, and a small card has been led which second player does not cover with a card higher than your honour, you must in every case play the honour. But if ten is led, and not covered by Knave, you would not play the Queen, but pass the ten; for, as already explained, you gain nothing by playing the Queen if the lead is from King, Knave, ten, while, if the ten is a strengthening card, you lose all command of the suit if you cover. If you hold Knave, nine, and others, and eight is led, you may finesse the nine or pass the eight; eight can hardly be played as a strengthening card, nor from a three-card suit, containing both Ace, King, or King Queen, for from either of these combinations the proper lead is the King. If the eight is the lowest of a four-card suit, the suit can be no other than Ace, Queen, ten, eight. If the lead is from a three-card suit, the eight being the lowest, the other cards may be Ace Queen, Ace ten, King ten, or Queen ten. The ten can therefore only be to your left in the single case of the lead being from Ace Queen eight—which is an unlikely lead anyhow, though, of course, possible, since leader may hold besides four trumps, and two weak three-card suits. You are therefore, in all probability, quite safe in finessing the nine or passing the eight according as you may wish the lead to lie (supposing the trick to fall to the eight or nine).

When, third in hand, your best card is small, you play it, of course, if it is higher than the card played on your right, or higher than and not in sequence with the card led. If you cannot cover, and both the cards already played are small, remember that your card tells both your partner and your adversaries of extreme weakness, both in individual cards and numerically. In such cases the question may arise sometimes whether it may be better to deceive the enemy or not by playing a false card. For instance, if your partner leads four, second hand plays six, and you hold three and five; if you play the five, it will be thought that you do not hold any other card in the suit. Cases may arise where it may be more important to cause the adversaries to suppose this erroneously, than to leave them and your partner in doubt by playing the lower card. It is worth your while in every such case to consider which is likely to be the more advantageous course.

The play of third in hand, second round of a suit, depends generally on the fall of the cards. As a rule, you know pretty well how the cards lie at this round, whether the original lead were your partner's or your own. In the former case you have the indications from the original lead, as already explained; those from the play second and fourth in hand, those from the renewed lead, and play second in hand second round, besides your own original hand. In the latter, as the suit is presumably your longest, you have rather more information in your own hand than in the other case, and rather less from the play; but if you have been watchful you have usually learned a good deal. Thus, suppose, having King and three other clubs 10, 4, 3, you lead the 3, and the cards fall thus,—

C 3 C 8 C Q C 6

you know that, apart from signalling for trumps, your partner must hold the two and five, second player has nothing below the eight, fourth player nothing below the six. Now, suppose that on the return of the suit your partner leads the two; then, as this is not the highest left in his hand, you know that besides the five he has one other, which is not the Ace, for if he had had the Ace he would have led it. Nor can it be the Knave, for if he had had Queen Knave, he would have played the lower of the sequence. You hold the King yourself, therefore his remaining card must be either the seven or the nine. You know this before second hand has played to second round. You know also that second hand has not the Ace, or he would not have allowed the Queen to take the first trick. Suppose, now, second hand plays the nine. Then the only card whose position remains doubtful is the Knave; it may be to your right or to your left. But you can play with as much confidence as if you knew where it was. For if it lies to your left along with the Ace, which certainly lies there, you lose nothing by finessing the ten; and if it lies to your right you gain a trick—apart from ruffing.

Take again the following case,—

Your hand being Q, Kn, 5, 2 of Hearts (trumps), the first round is—

| *A.* | *Y.* | *B.* | *Z.* |
|---|---|---|---|
| H 2 | H 9 | H K | H 6 |

Second round, to second in hand, is let us say— H 8 H 7

You know certainly that either four, or three, and ten lie with *Y*, and no more. For three and four are certainly not both with *B*, or he would have returned his lowest. Neither four nor three lie with *Z*; therefore, one of these cards lies with *B*, who has, therefore, led the best of two. You can thus place every card, assuming always that all are playing according to the customary rules for good play. *B* has either the three or four left. *Y* has ten, and either four or three. *Z* has the Ace, and is holding it back for some purpose connected with the strategy of his hand. You therefore play Knave third in hand; whereas, had you not attended to the fall of the cards, you might have thought it a fair finesse—only ten and Ace being against you, and the Ace probably in fourth hand—to pass the eight, so as to make sure of taking the last round in trumps. As it is, you know that, after your Knave has taken the second round, your third lead of the five will draw both the Ace and ten, leaving you with the long trump.

A BUMPER AT WHIST.—A correspondent, "Hazard," asks how *to determine the odds against a bumper at Whist?* The problem is a very difficult one. In fact, it is practically insoluble. Our correspondent remarks that there are nine ways of winning a rubber of two games, meaning, we suppose, that each game may be won, either by five or more points, alone; by three or more points and two by honours; or by one point or more and four by honours; and therefore the pair of games in $3 \times 3$ different ways. But, in reality, there are many more ways than three in which a game can be won, even if we only consider the points obtained, and the honours marked. And to determine the true chances we must consider more than this. We should have to determine the number of combinations of hands by which with the best play, or with good play on both sides, or with moderate play met by bad play, or with good play met by atrocious blundering, and so forth, a game might be made in a single round; in how many ways it might be made under correspondingly varying conditions in two rounds, in three rounds, in four, and in five. The same would have to be done for the second game. The numbers thus obtained would have to be multiplied together, or rather the number squared. Then the total number of combinations for all cases of one round, two rounds, &c., for each game would have to be determined. The chance of a bumper would be the ratio of the former number to the latter. But no one could possibly work out the problem in this way.—EDITOR.

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